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Summary of the XXX Rencontre de Moriond QCD Session

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Abstract

The main topics covered in this summary talk are the Large Hadron Collider, heavy ion collisions, renormalons, parton distribution functions, measurements of the strong coupling and the top quark. Of special interest this year was the discovery of the top quark by the CDF and D0 groups at Fermilab. I conclude that the evidence is compelling and that the best fit of the Standard Model top hypothesis to the data gives a top mass of 170 ± 9 GeV with a good χ^2.

Introduction

This session of the Rencontre de Moriond was packed with exciting new physics results, which were presented in approximately 110 talks. In addition, there were lively discussions after each talk. It is, of course, hopeless to summarize all of this in a one hour talk. Indeed, most of the talks were themselves summaries. What I try to do is to outline some of the main themes. In addition, I supplement what is contained in the individual talks by providing some commentary on particular subjects, particularly points raised in discussions, results that some participants found surprising, or ideas that seemed to thread through several talks. I also present some suggestions for future theoretical efforts.

In this summary, I refer to the talks contained in these proceedings by simply giving the author's name in italics. References to the original literature are mostly provided in the individual talks.

Of course, I must omit many topics. For example, I am excited by the results of the H1 and Zeus collaborations at HERA concerning the diffractive structure function F_2^{diff} in deeply inelastic scattering. As outlined in the talk of *T. Doeker*, these groups have measured deeply inelastic scattering in events where the incoming proton is diffractively scattered, exchanging a Pomeron with the rest of the system. (The scattered proton is not detected in the experiments reported, but one observes that the forward moving debris that normally results from the breakup of the proton is absent in these events, leaving a "rapidity gap.") Essentially, this is the Rutherford experiment with the Pomeron as the target, and one finds that the Pomeron is made of pointlike partons.

I should also note that the recent improved measurement of the mass of the W boson was covered in the electroweak session of the Rencontre de Moriond.

Prospects for the Large Hadron Collider

The Large Hadron Collider was approved this year, providing a path for experimental investigation of physics on the TeV scale, and, in particular, of the physics of electroweak symmetry breaking. As reviewed in a talk by *J. Aubert*, there are two general purpose detectors planned, CMS and Atlas, plus a detector, Alice, for investigation of heavy ion collisions. The design and prototype testing for these detectors is underway.

The prospects for finding the Standard Model Higgs boson (if that is what generates electroweak symmetry breaking) were reviewed by *L. Poggioli*. He reported on detailed studies with full detector simulation. If the Higgs mass is below about 150 GeV, the strategy is to look for its decay into two photons, and the search will be difficult. For a heavier Higgs, one will look for decay to two gauge bosons which in turn decay into four leptons. Here the search is easier. For a Higgs mass above some 700 GeV, the Higgs boson is a very broad resonance and its production cross section is small. Here, one will have to look for decays to two leptons and two hadronic jets. The prospects for finding supersymmetric Higgs bosons were discussed by *A. Nikitemko* and *D. Graudenz*.

Heavy Ion Collisions

Results from heavy ion collisions were reported by *I. Tserruya, G. Paic, T. Alber* and *L. Gutay.*

The eventual goal is to investigate the quark-gluon plasma that may form in high energy, head-on collisions of large nuclei. This is, however, not so easy, since one must have a good probe of the plasma dynamics, but the particles that one can observe in the final state mostly arise from material that has cooled and returned to being ordinary hadronic matter.

An analogy may be useful. There once was (we believe) a quark-gluon plasma, and it filled the entire universe. If this quark-gluon plasma had emitted weakly interacting probe particles that our eyes could see, then we could see it by looking at the night sky. Of course, this plasma disappeared some twelve billion years ago, but our imaginary probe particles would still bring us the evidence after a twelve billion year trip. Indeed, if we look at the night sky with detectors of microwave photons, we do see a plasma. It is, however, an electron-nucleus plasma, not the quark-gluon plasma. The photons are left over from the time that the electrons combined with the nuclei to form atoms. In a sense, we can also observe the hadron gas that existed in a certain era between that of the quark-gluon plasma and the electron-nucleus plasma. That is because the ratio of H to He nuclei was frozen during a certain minute of the dynamics and, not subject to the deadening laws of equilibrium thermodynamics, has been largely unchanged to this day.

It may prove easier to probe a man-made quark gluon plasma than it is to see the one made by nature twelve billion years ago. One probe is the ratio of strange to non-strange particles, which is analogous to the H to He ratio mentioned above. This was touched on in the talk of *C. Merino.* Another possible probe is provided by J/ψ and ψ' particles, which melt in a quark gluon plasma, as discussed by *D. Kharzeev.* A third probe is high transverse momentum jets, which, according to the talk of *S. Peigné*, cannot easily penetrate a quark-gluon plasma. It remains to be seen whether any of these probes can provide a conclusive signature for the transient formation of a quark-gluon plasma in heavy ion collisions.

B Physics

We heard results on B decays to charm as measured at LEP from *D. Koetke*, on semileptonic B decays measured by CLEO from *D. Cinabro*, on B masses and lifetimes as measured at LEP from *V. Canale*, on B mixing, lifetimes and rare decays as measured by CDF and D0 by *D. Lucchesi*, on B oscillations as measured at LEP by *S. Emery* and *L. Brillault*, on B decay to baryons as measured by CLEO from *M. Zoeller*, on polarization in B decays to ψK^* as measured by ARGUS from *D. Ressing*, on B^* and B^{**} states seen at LEP from *S. Schael*, on $B\pi$ and BK correlations observed by OPAL from *C. Sheperd-Themistocleo* and on Λ_B production as measured by L3 from *M. Lenti.* In addition, there were results on inclusive b production in hadron collisions as measured by CDF and D0 presented by *L. Markosky* and *V. Papadimitriou.* It is certainly heartening to see so much progress in one year. I cannot help but take note of a difference between this year's meeting and previous Rencontres de Moriond that I have attended: the emergence of LEP and Fermilab as B-factories with the help of silicon vertex detectors.

Let me emphasize one anomaly among the results reported. According to our best understanding of how field theory and QCD operates, the lifetime of a hadron containing a single heavy quark Q of mass m_Q should be given by

$$\tau = \tau_{\text{parton}}\{1 + \frac{\kappa^2}{m_Q^2} + \cdots\},\tag{1}$$

where τ_{parton} is independent of which hadron is decaying and κ^2 is a matrix element of certain operators in the hadron state. The most important operator is related to the kinetic energy of the light parton degrees of freedom. (*Cf.* the talk of *A. Vainshtein.*) The power corrections above begin at $1/m_Q^2$; there are supposed to be no $1/m_Q$ terms, although one could worry that effects of "ultraviolet renormalons" might produce a $1/m_Q$ contribution. If we believe this formula for b quarks with a mass $m_b \approx 5$ GeV, then all hadrons containing one b quark should have approximately equal lifetimes, with differences of order $(\kappa/m_Q)^2 \sim (0.5 \text{ GeV}/5 \text{ GeV})^2 \sim 0.01$. The results from LEP comparing the two B mesons are in accord with this expectation: $\tau(B^+)/\tau(B^0) = 1.08 \pm 0.08$. However, the result $\tau(B^+)/\tau(\Lambda_B) = 1.48 \pm 0.13$ creates a puzzle.

Another anomaly concerns the total rate for production of b quarks in $p\bar{p}$ collisions, as measured by D0 and CDF. The b quarks must have a transverse momentum bigger than some value $P_{T\min}$. The experimental rate is above the QCD prediction at next-to-leading order for all values of $P_{T\min}$, from 6 to 40 GeV. The results were presented as being in agreement with QCD theory, but the disagreement is typically some 30%. It seems to me that theorists should be able to do better than that, particularly when a P_T scale of 40 GeV is involved. I would like to suggest that for large $P_{T\min}$ it would be useful to use a theoretical formulation in which $P_{T\min}$ provides the hard scale. A function $f_{b/p}(x,\mu)$ that gives the distribution of b quarks in a proton and a function $d_{b/a}(x,\mu)$ that gives the distribution of b quarks in the decay of a light parton a appear in this formulation. These functions can be calculated from QCD theory, but in a calculation with a hard scale that is only m_b instead of $P_{T\min}$. It would be interesting to determine these functions from the data. If they differ somewhat from what calculation says they should be, it would be both interesting and useful for other applications to know what the discrepancy is.

A third anomaly that was discussed at this meeting concerns the production of J/ψ, ψ' and Υ in $p\bar{p}$ collisions. The rates for these processes have been problematic for QCD theory. A more sophisticated theory seems to help, as reported by *M. Cacciari.* However, the problems did not appear to be completely solved. I would like to point out that the theoretical problem facing those who attempt to calculate these decay rates is not simple, since the heavy quark mass m_Q is not the only hard scale. The inverse size of the quarkonium wave function $\sim \alpha_s m_Q$ also enters the problem, and this scale is not so hard. In my estimation, this field is making theoretical progress, and one should not be discouraged if it is not completely sorted out yet.

Finally, the CLEO Collaboration reported the observation of the decay $B \to \pi e \nu$. This decay involves a quark decay $b \to u$. One would like to disentangle the weak from the strong interactions so that such a measurement of a weak decay of a hadron gives the value of the

weak mixing matrix element V_{ub}. A good way to do this is to use lattice QCD to calculate the hadronic part. For this purpose, the decay $B \to \pi e \nu$ has a special significance, since the lattice method is best adapted to use in an exclusive decay with a simple final state. Transitions $b \to u$ have been observed before, but this is the first exclusive $b \to u$ decay that has been measured.

Renormalons

I now turn to a theoretical subject. Renormalons were discussed in the talks of *V. Braun* and *G. Korchemsky*, but they also entered obliquely into other talks and into the discussions. Many Moriond participants may have wondered what this discussion meant.

Partly, what may seem to be a shift of paradigm among QCD theorists is only a change of jargon. In a typical QCD expansion for a physical quantity,

$$R(\alpha_s(Q^2)) = 1 + A_1 \left(\frac{\alpha_s(Q^2)}{\pi} \right) + A_2 \left(\frac{\alpha_s(Q^2)}{\pi} \right)^2 + \cdots + \frac{m^2}{Q^2} + \cdots, \qquad (2)$$

one has perturbative α_s^N terms that fall off like logarithms of the hard scale Q^2 and one has power suppressed terms that fall off like powers of Q^2. Theorists used to denote the power suppressed terms by using the obscure technical term "higher twist." The new jargon is "renormalon terms."

There is, of course, a technical meaning, just as there was for "higher twist." The word "renormalon" refers the Borel transform $\tilde{R}(z)$ of the physical quantity $R(\alpha_s)$ and to a certain kind of singularity of $\tilde{R}(z)$ in the complex z plane. There are infrared and ultraviolet renormalon singularities. Here I consider the infrared renormalons, which are the most dangerous.

The physical interpretation of these singularities is as simple as it is significant. Consider a graph contributing to, say, the conventionally normalized cross section for $e^+ e^- \to$ hadrons, $R(\alpha_s(Q^2))$, where the photon virtuality Q^2 is large. Suppose that the graph contains a gluon with momentum k^μ. The contribution to such a graph from the integration region in which k^2 is smaller than some hadronic mass m^2 is small. In this example, it is of order m^4/Q^4. Thus we normally don't worry about such contributions and we perform the integration using a perturbative gluon propagator. However, ultimately the contributions from this integration get out of control. In fact, if we dress the gluon propagator with gluon loops, then at order α_s^N the perturbative integral is dominated by the region $k^2 \sim Q^2 e^{-N}$, giving a badly behaved contribution of order $\alpha_s^N \times N!$. Clearly if we try to go beyond a calculation of order $N \approx \log(Q^2/m^2)$ then the dominant integration region is just the region $k^2 < m^2$ where perturbation theory should not apply.

Thus contributions from infrared integration regions in Feynman graphs are connected to an $N!$ growth of the value of certain kinds of high order graphs and to power suppressed "infrared renormalon" contributions to physical quantities. The power suppressed contributions are of practical importance in the case of τ decay. To estimate the size of the power suppressed contributions, one replaces the propagator for the low momentum gluon

by a phenomenologically determined vacuum matrix element of a gluon operator, using the formalism of the operator product expansion.

Parton Distribution Functions

One ingredient in QCD calculations involving hadrons is the parton distribution functions. Thus the measurement of these functions is an important goal. In addition, the consistency of measurements in a variety of processes and at a variety of momentum scales provides a check on the QCD theory. *R. Roberts* reviewed parton distributions for the conference. There were talks on a variety of measurements that bear on the determination of these functions. *M. Klein* covered the measurement of $F_2(x, Q^2)$ in deeply inelastic scattering at HERA, which I discuss briefly below. *A. Kotwal* discussed measurements of $F_2(x, Q^2)$ on protons and deuterons by the E665 experiment (see also the review of *B. Badelek*). *M. Szleper* covered measurements by the NMC collaboration of $F_2(x, Q^2)$ on ^{119}Sn and ^{12}C. There were three talks reporting measurements by the D0 and CDF collaborations of cross sections in $p\bar{p}$ collisions that provide information on parton distributions. *Q. Fan* reported on the cross section for W production, which bears on the ratio of the number of up quarks to the number of down quarks in the proton. These measurements helped catalyze changes in published parton distributions last year. *J. Lamoureux* talked about the cross section for γ production in $p\bar{p}$ collisions, which provides information on the gluon distribution. Finally, *T. Geld* reported cross sections for dijet production, which also provides information on the gluon distribution.

Before proceeding to a discussion of individual reactions, I offer two general comments. First, it became apparent more than once during the discussions that the particle physics community could make good use of parton distributions that provide the best fits to the world data for a variety of choices of α_s. This is easy, and has in fact been done from time to time. The MRS group provided a set a few years ago in which there were a variety of choices for α_s and the shape of the gluon distribution.[1] Last year, the CTEQ group provided a standard set CTEQ2M and an alternative set CTEQ2ML in which α_s was set to the value determined by LEP experiments.[2] There is also a need for published parton distributions that come with an error matrix. Then, given a calculation of a cross section, one could assign an error attributed to uncertainties in the parton distributions. This is not easy, and it has not been done. The best currently available method for assigning an error attributed to uncertainties in the parton distributions is to try the calculation with two or three published parton sets and take the difference in the results as an error estimate. This is similar to estimating the size of a French mountain valley by taking the r.m.s. dispersion in the locations of individuals in a flock of sheep grazing in the valley.

I now turn to the measurement of $F_2(x, Q^2)$ by the H1 and Zeus collaborations at HERA. Since HERA has a large reach toward small x with still substantial Q^2, the greatest interest here has been the small x region. The results of both groups appear to be in good agreement. Before the experiments were done, there had been an expectation that $F_2(x, Q^2)$ would rise at small x. This expectation was based, on one hand, on the BFKL equation for the variation of F_2 as a function of x. On the other hand, it was based on the evolution equation for the

variation of F_2 as a function of Q^2: if F_2 were flat as a function of x at a low value of Q^2, it would quickly develop a slope at larger values of Q^2. The experimental result is that $F_2(x, Q^2)$ does rise at small x, although not as fast as predicted by the lowest order BFKL equation.

The Q^2 variation of $F_2(x, Q^2)$ provides information on the gluon distribution, since in the evolution equation for $\partial F_2/\partial Q^2$ the right hand side contains a term proportional to the gluon distribution. When the gluon distribution is thus determined, the normal 2-loop Altarelli-Parisi evolution equation appears to work well, despite earlier speculations that one might see the breakdown of this equation at small x. This equation can be improved with a summation of leading $\log(1/x)$ terms, as discussed by *F. Hautmann, R. Ball*, and *R. Peschanski.*

The reaction $p\,\bar{p} \to \gamma\,X$ plays an important role in determining the gluon distribution. A problem with this determination was reported in the talks of *R. Roberts* and *W. Vogelsang.* There are experiments at a variety of values of $\sqrt{s}$ and in each such experiment the observed cross section falls faster with the transverse momentum P_T of the photon than is predicted by the theory using the parton distributions that give the best overall fit to this and other data. *G. Korchemsky* presented a solution of this problem based on smearing the P_T distribution of the photon[3] with transverse momentum generated by the emission of multiple soft gluons from the incoming partons.

P. Grenier and *J. Saborido* presented results concerning polarized parton distributions. These parton distributions, as measured by the structure function $g_1(x, Q^2)$, should obey a certain sum rule due to Bjorken. Analyses of data from experiments E143 and E142 at SLAC and from the SMC collaboration indicate that the Bjorken sum rule holds within the experimental errors.

Finally, *J. P. Guillet* reviewed work on parton decay functions $d_{A/a}(z, \mu^2)$, while new data useful for their determination were presented by *Y. Yamada* (TRISTAN), *M. Watson* (LEP) and *K. Baird* (SLD). The parton decay functions are just as fundamental as the more familiar parton distribution functions $f_{a/A}(x, \mu^2)$ — or just as non-fundamental, depending on your view. They are not as important as distribution functions, which are essential for every QCD experiment with hadrons in the initial state. Nevertheless, parton decay functions are still of substantial practical usefulness, and in my opinion it is good that we are now determining them from data.

Measuring the Strong Coupling

The measurement of α_s is not the sole goal of QCD studies, but it is significant as a determination of one of the fundamental constants of nature and as an input for studies of what may lie beyond the standard model. In addition, the agreement among measurements made with different methods and at different scales provides a check on the correctness of the theory. My impression from the results presented at Moriond XXX is that the level of agreement is not quite consistent with the expected experimental and theoretical errors.

One needs a careful definition in order to compare results, since the renormalization method affects the meaning of α_s. Fortunately, there is a consensus to adopt as a standard

of comparison $\alpha_s(\mu)$ defined in the $\overline{\text{MS}}$ scheme with five flavors, choosing $\mu = M_Z$. Other measurements, say measurements of $\alpha_s(\mu)$ with three flavors at $\mu = M_\tau$, are translated to this standard.

The talks on measurements of α_s were reviewed by *S. Betkhe*. He reported a value $\alpha_s(M_Z) = 0.117 \pm 0.006$ based on the previous world average together with the results reported at this meeting. I review below a few of the new results.

The measurable quantity associated with the smallest theoretical errors on α_s is the width for $Z \rightarrow$ hadrons at LEP. The perturbative expansion is known to next to next to leading order, the perturbative coefficients appear to be well behaved, and the power suppressed corrections are negligible. Unfortunately, the theoretical expression has the form $\Gamma = \Gamma_0\{1 + (\alpha_s/\pi) + \cdots\}$, so that experimental errors in the determination of Γ are magnified when expressed as errors on α_s: $(\delta\alpha_s/\alpha_s) \approx (\pi/\alpha_s) \times (\delta\Lambda/\Lambda) \approx 30(\delta\Lambda/\Lambda)$. Despite the difficulties, there has been progress over the years. The value reported twelve years ago at the 1983 Multiparticle conference was[4] $\alpha_s(M_Z) = 0.153 \pm 0.050$. The LEP result reported by *J. Casaus* at this conference was $\alpha_s(M_Z) = 0.127 \pm 0.006$. Thus the error has been reduced by an order of magnitude in twelve years. Note that the value obtained is a bit high compared to the world average value.

Results were also reported for event shapes in $e^+e^- \rightarrow$ hadrons. Here the purely experimental errors are small, but the theoretical error is large: there are only two terms known in the perturbative expansion, the indications based on scale dependence are that the perturbative expansion is not so well behaved, and estimated power suppressed corrections are substantial. Still, we are better off than we were in 1983, when the state of the theory was such that values that were not consistent with one another were obtained. For instance[4], one analysis gave $\alpha_s(M_Z) = 0..165 \pm 0.010$ while another gave $\alpha_s(M_Z) = 0.119 \pm 0.010$. At this meeting, *K. Baird* reported results from SLD of $\alpha_s(M_Z) = 0.120 \pm 0.008$ and *J. Casaus* reported results from LEP of $\alpha_s(M_Z) = 0.123 \pm 0.006$. Within the errors, these are consistent with the world average.

A measured quantity that is closely related to $\Gamma(Z \rightarrow$ hadrons$)$ is the width for $\tau \rightarrow \nu +$ hadrons. The difference here is that the scale is M_τ instead of M_Z. Since M_τ is so small, one wonders whether measurements of $\Gamma(Z \rightarrow$ hadrons$)$ can provide a credible measurement of α_s. In particular, the power suppressed corrections are not negligible. The indications from careful analyses is that the measurement is credible[5], but some authors[6] believe the theoretical error on $\alpha_s(M_Z)$ is as small as ± 0.002, while others[7] believe that it is no smaller than ± 0.006. In my opinion the distinction is between theorists who quote a "1 σ" error, with the meaning that they expect that the error is not much smaller than the estimate given, and theorists who quote a "95% confidence limit," with the meaning that they are pretty sure that the error is smaller than the estimate given. It seems to me that ± 0.003 is a reasonable estimate as a 1 σ error, while a 95% confidence limit might be ± 0.010. The LEP values presented at this conference by *P. Reeves* were $\alpha_s(M_Z) = 0.123 \pm 0.003$ from OPAL and $\alpha_s(M_Z) = 0.122 \pm 0.003$ from ALEPH. *D. Dumas* reported the result $\alpha_s(M_Z) = 0.114 \pm 0.003$ from CLEO. *P. Raczka* presented a theoretical analysis based on previous data that gave a value $\alpha_s(M_Z) = 0.120 \pm 0.003$. It is not clear to me where the differences among these

results come from.

The value of α_s can also be extracted from deeply inelastic lepton scattering. Global fits to parton distributions produce a fitted value of α_s, but it is difficult to determine the corresponding error. An evaluation based on the QCD corrections to the Gross-Llewellyn Smith sum rule and data from the CCFR collaboration was presented by *D. Harris*: $\alpha_s(M_Z) = 0.107^{+0.007}_{-0.009}$. Note that this result is below the world average value.

The final example that I will discuss is based on the level splittings of $c\bar{c}$ and $b\bar{b}$ states, which can be very well measured. The idea is to calculate these splittings with lattice QCD and adjust the lattice α_s to match the observed splittings. One must correct for the facts that the lattice spacing is not zero and the lattice size is not infinite. In previous years this calculation was performed with the number of light quark flavors set to zero, and one had to correct for the $N_f = 0$ approximation. This year there are new results with $N_f = 2$, so this correction is smaller. Finally, one must relate the five flavor, $\overline{\text{MS}}$ version of α_s at $\mu = M_Z$ to the version of α_s used on the lattice. This requires a perturbative calculation. The result[8] reported by *P. Mackenzie* is $\alpha_s(M_Z) = 0.115 \pm 0.002$. This is a new result and may take a year to settle down, but it appears to me that this method will set the state of the art in reliability and precision.

Discovery of the Top Quark

It was been a great joy to hear from the CDF and D0 collaborations the details of their discovery of the long awaited top quark. The information was presented in talks by *A. Yagil* and *J. Thompson*.

I note first that top quark production is a short distance process, which should be calculable in perturbative QCD. For instance in the Born graph for gluon + gluon $\rightarrow t + \bar{t}$, there is a virtual top quark exchanged. The virtuality $|k^2 - M_t^2|$ of this line is of order M_t^2 or larger, so the virtual top quark is far off shell. We thus expect the cross section calculated at next to leading order to be quite accurate.

The two experimental groups used a number of different methods for tagging events as candidates for top quark events. I provide here a quick overview, with the warning that the reader should consult the full talks for a more complete description.

First, both groups looked for decays of the produced $t\bar{t}$ into two leptons plus jets. The idea is that each t decays to Wb and the W decays into a charged lepton plus a neutrino. (I group here the dilepton analysis of CDF and the $e\mu$ + jets, ee + jets and $\mu\mu$ + jets channels of D0). I show below for each detector the expected background, the expected signal plus background based on a 170 GeV top quark, and the observed number of events.

$$\ell\,\ell + \text{jets}$$

	background	top+bkg	observed
CDF	1.3	4.4	7
D0	0.7	2.3	3

Evidently, the hypothesis that there is a 170 GeV standard model top quark fits the data much better than does the hypothesis that there is only background. Next, both groups

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looked for decays of the $t\bar{t}$ into a single isolated lepton plus jets where one of the jets contained a muon, presumably from a b quark decay. (I group here the SLT analysis of CDF and the $e+\text{jets}/\mu$ and $\mu+\text{jets}/\mu$ channels of D0.) The results were

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$\ell+\text{jets}/\ell$

	background	top+bkg	observed
CDF	15	22	23
D0	1.2	3.5	6

</div>

Again, the results favor the top quark hypothesis. The D0 group also looked for a single charged lepton plus jets without another muon to tag a b quark decay, but with more stringent requirements on the jets. (I group here the $e+\text{jets}$ and $\mu+\text{jets}$ channels of D0.) The results were

<div align="center">

$\ell+\text{jets}$

	background	top+bkg	observed
D0	1.9	6.4	8

</div>

Again, the top hypothesis is favored. Finally, CDF looked for events with a lepton plus jets in which they could find the secondary vertex from the b quark decay in their silicon vertex detector. The results were

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$\ell+\text{jets}$ [secondary vertex]

	background	top+bkg	observed
CDF	7	24	27

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Thus the top quark hypothesis is favored in several different methods of analysis. Furthermore, the expected number of events matches the observed number pretty well, although there is a tendency for the observed number to be greater than the expected number.

Both experimental groups give a top quark mass analysis, for which I refer the reader to the groups' talks.

It remains for future experimental work to test whether the object that is seen is precisely the top quark of the standard model and not some variant of that particle. We will want to pin down, for instance, the angular distribution with which top quarks are produced, the branching ratios for its various decay modes, and the momentum distributions of its decay products.

For now, however, I would like to use the information at hand to address the question of the top quark mass. I will assume that the D0 collaboration is right, within its stated errors. I will assume that the CDF collaboration is right, within its stated errors. I will assume that the object found is indeed the standard model top quark. And I will assume that the QCD theory that predicts its production cross section[9] is right, within errors. The question then is, what is the top quark mass and what is its production cross section?

The CDF Collaboration quotes a mass of (176 ± 12.8) GeV, where I have combined the statistical and systemic errors. They quote a cross section of $6.8 \; ^{+3.6}_{-2.4}$ pb. Thus if m is the

mass in GeV and σ is the cross section in pb, I assign a χ^2 from the CDF measurement of

$$\chi^2_{\text{CDF}} = \left(\frac{m - 176}{12.8}\right)^2 + \left(\frac{\log(\sigma/6.8)}{0.307}\right)^2.\tag{3}$$

(I rather arbitrarily take the errors to be Gaussian in the logarithm of the cross section, rather than the cross section itself.)

The D0 Collaboration quotes a mass of (199 ± 29.7) GeV, where I have again combined the statistical and systemic errors. D0 shows a curve, with an error band, for the cross section as a function of the mass of the top quark. Reading from their figure, I find that in the region near $m = 175$ their central value for the cross section is $\log(\sigma) \approx \log(8.7) - 0.0117(m - 175)$, with an error on $\log \sigma$ of about 0.338. Thus I assign a χ^2 from the D0 measurement of

$$\chi^2_{\text{D0}} = \left(\frac{m - 199}{29.7}\right)^2 + \left(\frac{\log(\sigma/8.7) + 0.0117(m - 175)}{0.338}\right)^2.\tag{4}$$

Finally, I read the theoretical cross section from the curve shown in the D0 paper as $\log(\sigma) \approx \log(5.0) - 0.0322(m - 175)$. I rather arbitrarily assign a 20% error to this, based on the belief that the parton distributions that go into the calculation are not known to better than 10% and the cross section is proportional to the products of two parton distributions. This amounts to an error of $\log(1.2) = 0.182$ on $\log \sigma$. Thus I assign a χ^2 from the theoretical prediction of

$$\chi^2_{\text{T}} = \left(\frac{\log(\sigma/5.0) + 0.0322(m - 175)}{0.182}\right)^2.\tag{5}$$

It is now a simple matter to choose m and σ so as to minimize the total χ^2. The minimum χ^2 is

$$\chi^2_{\text{min}} = 2.5.\tag{6}$$

This is a very reasonable value for three degrees of freedom (five contributions to χ^2 minus two parameters fit.) The individual contributions are $\chi^2_{\text{CDF}} = 0.2$, $\chi^2_{\text{D0}} = 2.0$, and $\chi^2_{\text{T}} = 0.3$.

The fitted value for the top quark mass is

$$m = (170 \pm 9) \text{ GeV},\tag{7}$$

while the cross section is

$$\sigma = 6.5 \, {}^{+1.9}_{-1.5} \text{ pb}.\tag{8}$$

The mass value is lower than that quoted by either CDF or D0. The reason is that the mass is partly determined by matching the observed cross section to the theoretical cross section, which decreases rather sharply with increasing mass.

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